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Abstract

This paper adds new empirical evidence to the political economy literature of economic reform. One of the main contributions of this paper is the development of a novel methodology to identify privatisations. The methodology is a combination of the Bai & Perron structural break filter, and validation of the breaks identified by this filter using *de jure* evidence of reforms. 21 *de facto* healthcare financing privatisations are identified in a sample of 23 OECD countries. It is analysed which factors trigger or hinder these privatisations. Robust evidence is found in favour of the 'crises induce reform hypothesis'. That is, severe economic recessions, high levels of unemployment and high interest rates on government debt trigger privatisations. Contrary to theory and conventional wisdom, robust evidence is found that political factors do not have an impact. Ideology, government or political fractionalisation, or major cabinet changes are not found to significantly affect the likelihood of healthcare financing privatisations.

Keywords: Healthcare privatisation, Reform measurement, Political economy, OECD countries

JEL codes: H51, P16

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1. Introduction

This study quantitatively analyses the political and economic determinants of healthcare financing privatisations, an endeavour not pursued previously. Given the economic magnitude of the healthcare sector this is a noticeable omission. In 2010 average public expenditure on healthcare in OECD countries was almost 8 % of GDP (OECD.org). A successful healthcare financing privatisation can reduce the fiscal burden of a country, improving the sustainability of government finances. Thus, this study is not only related to the literature on the determinants of privatisations (e.g. Bortolotti & Pinotti 2008; Roberts & Saeed 2012), but also to the literature on the determinants of fiscal adjustments (e.g. Alesina & Argdana 2012; Lavigne 2011). Healthcare privatisations are often a matter of political controversy. Hence, we expect that both political and economic factors drive privatisations. Therefore, it is asked *whether political and/or economic factors impact the likelihood of de facto healthcare financing privatisations in OECD countries?*

It is likely that research in this area is lacking due to the absence of a coherent methodology to identify gradual, but significant healthcare privatisations. A main contribution of this paper is the development of such a methodology. In the literature on economic reform at least two complementary views exist on which kind of data ideally should be used to measure reform. The first view, expressed in Campos & Horváth (2012), holds that economic outcome data and policy input data never should be mixed. The reason is that a failure to separate the two is likely to cause imprecise measurement of both timing and impact of reform. The second view, expressed in Rodrik (1996), holds that only policy input should be used to measure reform. The argument is that factors outside policymakers' control will impact economic data. Hence, only policy input data is a valid proxy for reform according to this view.

Here, a novel sequential approach is taken that utilises both economic outcome data and policy input data without mixing it. First, the Bai & Perron (1998, 2003) (B&P) structural break filter is applied to detect statistically significant privatisations. Then, the detected privatisations are validated using evidence of *de jure* reforms. Selecting one of the two steps alone when measuring reforms can lead to either: 1) Detection of changes that are not policy induced, and thus, do not qualify as reforms. 2) Identification of *de jure* reforms that did not have a significant impact, and thus, do not qualify as 'successful' reforms. The methodology detects *de facto* privatisations, that is, statistically significant policy induced shifts from public to private financing of healthcare.

Using this improved measure of reform allows us to rigorously test some of the usual suspects that are believed to determine whether economic reforms are initiated or delayed. To that end, the random-effects binary outcome logistic estimator is applied. Controlling for economic factors and duration dependence several interesting empirical results are found. First, the results suggest that severe economic crises trigger healthcare financing privatisations. Second, no evidence is found that pure political factors impact the likelihood of financing healthcare privatisations. These results are robust to different definitions of privatisation. Furthermore, the main findings are robust to different proxies for the hypothesised political determinants. Thus, the overall contribution of this paper is a coherent quantitative analysis of the determinants of *de facto* healthcare financing privatisations in OECD countries.

Section 2 develops the hypotheses while section 3 outlines the data and the empirical strategy. Section 4 presents the baseline empirical results and section 5 gives robustness analyses. The paper ends with a conclusion in section 6.

2. Hypotheses

In this section the hypotheses are developed. They are not derived from a single underlying theoretical model for healthcare financing reform because no such model exists. Instead, they are derived from related literature on the political economy of reform and privatisation.

Perhaps the most common view among scholars is that an economic crisis is a necessary and maybe sufficient condition to trigger reform. Nonetheless, the hypothesis has not been subjected thorough empirical testing. This lack of research can be due to several causes. First, when a hypothesis enters the realm of 'conventional wisdom' empirical testing often ends (Drazen & Easterly 2001). Second, Rodrik (1996) argues that the 'crises induce reform hypothesis' is a tautology, which means that there is nothing to test. "*Reform naturally becomes an issue only when policies are perceived not to be working. A crisis is just an extreme case of policy failure. That reform should follow crisis, then, is no more surprising than smoke following a fire*" (Rodrik 1996, p. 27). However, economic crises can take different forms and differences in the severity of crises can have a different impact on reform probability. For example, recessions may trigger healthcare reform while below average economic growth rates does not. Furthermore, the effect of crises in sparking reform may also depend critically on what sort of reform is being considered (Drazen & Easterly 2001). So, the hypothesis is falsifiable.

Two common explanations for the 'economic crises induce reform hypothesis' exist. The first is based on the presumption that when significant political opposition to reform exists, an economic crisis may overcome reform resistance by convincing the opposition that something needs to be done (Drazen 2000). The second is centred on the observation that ex-ante uncertainty about the economic outcome of reform often generates resistance to reform. An economic crisis raises the cost of not reforming, effectively decreasing political opposition (Fernandez & Rodrik 1991). These arguments lead to the first hypothesis.

H_1: The likelihood of healthcare financing privatisations increases when the economy is in a severe state of crisis.

Conventionally, right-wing market-oriented governments are thought to be inclined towards privatisation and liberalisation of public sector enterprises. In political economy, such an effect is typically referred to as the ‘partisan politics hypothesis’. To what extent do left- and right-wing politicians or parties provide policies that reflect the preferences of their electorates? According to the conventional approach right-wing governments implement policies that favour the preferences of relatively high-income voters, whereas left-wing governments implement policies that favour the preferences of low-income voters (Hibbs 1977; Alesina 1987). Empirical studies do not provide robust evidence for the hypothesis concerning privatisations (see Bortolotti & Pinotti 2008; Roberts & Saeed 2012). In fact, the empirical literature on the determinants of fiscal adjustments suggests that left-wing governments are likely to introduce reforms that normally are associated right-wing policies (see Tavares 2004).

This can be explained theoretically by the “Nixon goes to China” effect. Cukierman & Tommasi (1998) show that right-wing governments will find it harder to credibly signal that right-wing policies will be beneficial. The same holds for left-wing governments and left-wing policies. Governments are prone to become victims of their own ideology – only governments of the unexpected ‘political colour’ can credibly signal that a policy is expected to be beneficial. It is reasonable to expect that governments have private information about the highly uncertain economic outcome of healthcare privatisations. This asymmetric information is caused, for example, by access to expert knowledge. These arguments lead to the second hypothesis.

H_2: The likelihood of healthcare financing privatisations increases when a left-wing government is in office

The two previous hypotheses concern factors that trigger privatisations. However, it is interesting to investigate whether there are factors that diminish the probability of reform. It is often observed that welfare-enhancing policies are delayed. This can be explained by the ‘war of attrition hypothesis’, which state that political opponents play a ‘waiting game’. The basic insight from the Alesina & Drazen (1991) model is that governments will postpone unpopular reforms, even if postponement is sub-optimal for social welfare, until the ‘political’ costs of reform has fallen below the ‘political’ benefits of postponing it. This implies that more heterogeneous governments will be less keen to reform public enterprises because they have a more heterogeneous constituency to please. They will face higher costs, in terms pleasing their electorate, and hence, postpone reforms. Thus, large coalition governments will find it more difficult to agree on reforms compared to more homogeneous governments. Small groups can use their veto power to block reforms if the change in distribution of economic goods resulting from the reform will be too costly, in terms of preferences of their electorates. This argument also holds in the case of privatisations (Lora 2000). This explains the third hypothesis.

H_3: The likelihood of healthcare financing privatisation decreases when governments are more fractionalised

Lora (2000) point-out that the literature on the timing of reform offers little explanation of the apparent importance of one factor that seems to be the simplest reasons for reform, namely a change of government. Based on case studies, Haggard and Webb (1994) argue that a *window of opportunity* opens after elections. This can be exploited by newly elected governments, which "*typically enjoy a period in which the costs of adjustment can be traded against political gains*" (Haggard & Webb 1994, p. 8). Such a window of opportunity could, in relation to healthcare reform, be the interpretation made by a newly elected government that public healthcare expenditure have, or will become unsustainable unless significant efforts to curb the costs are initiated. This explains the fourth hypothesis.

H_4: The likelihood of healthcare financing privatisation increases when a newly elected government or government executive is in office

3. Data and empirical strategy

In this section the methodology used to detect healthcare-financing privatisations is presented. Furthermore, the independent variables and the econometric technique used in the estimations are discussed.

3.1 Dependent variable: Identifying privatisations

There exist two groups of definitions of privatisations in the literature, a broad and a narrow one. The broad definition concerns overall shifts in the boundary between public versus private involvement in the economic sphere (e.g. Vickers & Yarrow 1991). The narrow definition concerns shifts in ownership (e.g. Saltman 2003). The drawback of the broad definition is that it is problematic to operationalize. The drawback of the narrow definition is that it fails to capture shifts from the public to the private domain when no shift in ownership takes place. This is often the case with healthcare-financing privatisations (see appendix 1a). As shown below, we can measure public versus private sector involvement instead of ownership. And, as discussed in the introduction, we need to make sure that shifts in involvement are both policy induced and has a statistically significant impact. Therefore, a *de facto* healthcare financing privatisation is defined as: *A statistically significant policy induced shift from public to private sector financing of healthcare services*.

What we would like to measure is public versus private funds incurred to healthcare - from which pocket is healthcare expenditure paid, public or private? The historical ratio y_{it} of public healthcare expenditure relative to total healthcare expenditure (public + private) in country i at time t can be used to identify privatisations.[1] This ratio is calculated as: $y_{it} = \frac{publicfunds_{it}}{publicfunds_{it} + privatefunds_{it}}$. It can be interpreted as the percentage of public financing of total spending. Hence, we have a measure of public relative to private financing of heath care services.

Table 1: Descriptive statistics for variables used to construct the dependent variable

Variable	obs.	mean	st.dev.	min.	max.	source:
Public healthcare expenditure % of GDP	932	5.46	1.70	0.84	9.74	OECD.org
Private healthcare expenditure % of GDP	942	2.02	1.39	0.11	9.31	OECD.org
Total healthcare expenditure % of GDP (private + total)	931	7.50	2.26	1.49	17.67	Calculated
Public relative to total expenditure, y_{it}	931	0.73	0.13	0.22	0.98	Calculated

All available observations for the 23 OECD countries between 1960-2010 have been used. See table 2 for exact sample periods.

The idea, then, is to apply structural break testing to identify significant shifts in y_{it}. A structural break is the timing of a fundamental change in the Data Generating Process (DGP), for example as a result of an economic reform (Hansen 2001).[2] However, a structural break can be caused by other factors, such as an exogenous shift in consumer preferences or relative price movements. Thus, the detected structural breaks needs to be validated using *de jure* evidence of reforms. If a change in policy causes a significant part of healthcare financing to shift from (to) the public to (from) the private sector it is a *de facto* privatisation (nationalisation).

Perhaps the most well-known structural break test is the Chow-test. A noticeable feature of this test is that it is limited to test the hypothesis of whether a time series contains a single structural break. To use the test one has to split the sample at the point in time where *a priori* information leads one to suspect a break and then use F-tests to determine whether subsample parameters are significantly different. For the application at hand, *de jure* evidence gives *a priori* information of several structural breaks in each time series, up to 25 in some cases (see HSiT country reports). This means that the time series would have to be split into a large number of subsamples on which the Chow-test could be performed. This is infeasible because the time series are not long enough when a priori information leads us to suspect so many breaks. Furthermore, there is often a time lag before a *de jure* reform manifests itself in the data; institutions are rigid (Acemoglu et al. 2006). This means that the division of the time series into subsamples would be arbitrary.

The feasible approach is to start from the economic data and then use *de jure* evidence for validation. Hence, the number and timing of structural breaks are treated as unknown a priori. The econometric

1 Public healthcare expenditure is defined as: "*health expenditure incurred by public funds. Public funds are state, regional and local Government bodies and social security schemes. Public capital formation on health includes publicly financed investment in health facilities plus capital transfers to the private sector for hospital construction and equipment*" (OECD.org).
Private healthcare expenditure is defined as: "*Privately funded part of total health care expenditure. Private sources of funds include out-of-pocket payments (both over-the-counter and cost-sharing), private insurance programs, charities and occupational health care*" (OECD.org).

2 Seminal examples of structural breaks are: the unification of Germany, and the introduction of a common European monetary policy.

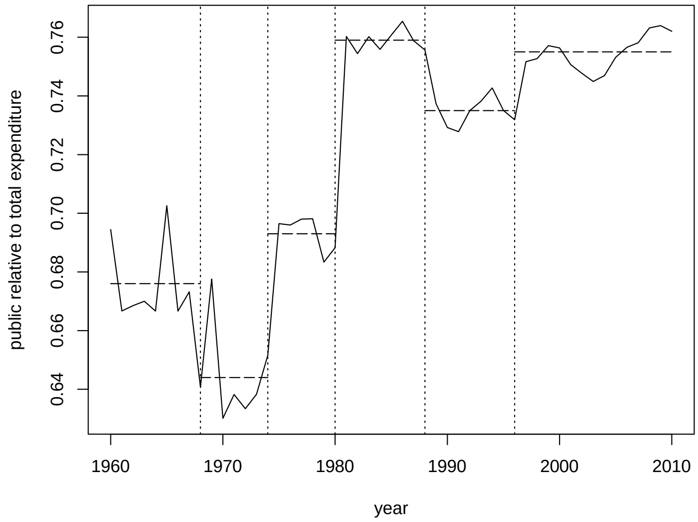
literature on detection of an unknown number of structural breaks of unknown timing is relatively sparse. Liu et al. (1997) suggest a method for pure structural changes, i.e. all parameters included are subject to changes. Bai & Perron (1998, 2003) develop a more general method for this purpose. Their method also allows the inclusion of parameters that are not subject to shifts, of which the pure structural change model applied here is a special case. The assumptions of the B&P-filter are less restrictive. Therefore, the B&P-filter is applied to identify structural breaks.

In order to define privatisations (and nationalisations) in the context of the B&P-filter consider a model with m possible structural breaks in an OLS regression framework that takes the form:

$$y_t = \delta_j + u_t \qquad (t=1,...,T\ ,\ \ j=1,\ldots,m+1)$$

Where y_t is the dependent variable, in this case the time series of public relative to total healthcare expenditure for each country considered. δ_j is a vector of estimated coefficients (constants) of which there are m+1, i.e. δ_j is the mean at the different segments of the time series y_t. u_t is the error term. The segments generate a stepwise linear route through the times series y_t and give m structural breaks. Fig. 1 provides a graphical exposition where the j=6 segments, or regimes are represented by dashed lines and the m=5 structural breaks are represented by dotted lines. A downward (upward) regime shift is detected as a potential privatisation (nationalisation), for which validation using *de jure* evidence is required. See appendix 1a for all time series of countries included.

Fig. 1: Structural breaks in healthcare financing source: The case of Austria



The data used to generate fig. 1 has 51 observations, the trimming parameter was set at h=5 i.e. at least 5 periods must pass between consecutive breaks. See main text for an explanation.

The idea underlying the B&P-filter is straightforward.[3] It generates the segmented route through the series that generates the lowest Sum of Squared Residuals (SSR) up to a maximum number of

[3] The process underlying the algorithm is straightforward. First, it searches for all possible sets of breaks up to a maximum number of breaks, restricted by the trimming parameter chosen, and determines for each number of breaks the set that minimises the SSR. Then a series of F-tests determine whether the improved fit produced by allowing an additional break is sufficiently

breaks. The maximum number of breaks is restricted by a trimming parameter h, which specifies a minimum number of observations that has to occur between consecutive breaks. In the context of fig. 1 the segments can be thought of as regimes where y_t fluctuates around the constant mean δ_j. A shift to a new regime is unlikely to happen by chance, dependent on the test-size employed. Thus, a regime shift implies that the underlying DGP has been altered generating a structural break.

When applying the B&P-filter several test procedures are possible (see Bai & Perron 1998, 2003 or Zeileis et al. 2003). Here the Bayesian Information Criterion (BIC) is chosen to select the optimal number of m+1 segments. Information criteria are often used for model selection, which in this case means selection of the m number of breakpoints. Bai & Perron (2003) argue that the Akaike Information Criterion usually overestimates the number of breaks, but that the BIC is a suitable selection procedure in many situations. Furthermore, when applying the B&P-filter a choice has to be made concerning the size of the trimming parameter h. If the times series do not exhibit autocorrelation or heteroskedaticity any trimming will work regardless of sample size (Bai & Perron 2003). However, when finite samples that do exhibit autocorrelation or heteroskedaticity are used the trimming needs to be increased. Here a trimming of h=5 is chosen because it generates the best fit with *de jure* evidence while still being econometrically sound. The outcome of using a more conservative trimming of h=6 can be found in appendix 1a. Autocorrelation and potential heteroskedasticity is modelled non-parametrically by running the filter using a Heteroskedasticity and Autocorrelation Consistent (HAC) estimate of the variance-covariance matrix.[4] Antoshin et al. (2008) show that HAC errors generally deal with autocorrelation better than parametric modelling.

The outcome of running the B&P-filter on 23 OECD countries can be found in column 3 in table 2. 1960-2010 is selected as sample period. For some countries data is unavailable for the whole period. In that case the longest data period available is used, see column 2 for exact sample periods.

It is possible that factors outside control of the policy-maker move the ratio significantly and hence look like a reform when it in fact was not. Therefore, it is checked whether they are likely to be the result of planned policy. See column 4 in table 2 for the outcome of this analysis, and table 1a in the appendix for a detailed description of the related *de jure* reforms.

In sum, the analysis reveals that 21 of the 33 detected privatisations can be validated using evidence of *de jure* reforms. We are therefore confident that these 21 structural breaks are the result of planned policy, and therefore match the definition of a *de facto* healthcare financing privatisation. Second, a

large, compared to what can be expected randomly, on the basis of the asymptotic distribution derived in Bai & Perron (1998). Starting with a H_0 of no breaks, sequential tests of k vs. k+1 breaks allow one to determine the appropriate number of breaks in a data series. Alternatively, the BIC (Bayesian Information Criteria) works well if there is evidence of at least one break. After determining the appropriate number of breaks the program extracts the corresponding break dates of the optimal sequential route. The trimming parameter h is expressed either as a fixed number of observations, or a percentage of the number of observations. Autocorrelation, trending time series and non-constant errors are permitted (Bai & Perron 2003).

[4] Choosing the trimming to be a fixed number of observations instead of a percentage of the sample size has the implication that the percentage of the sample size automatically is increased for shorter samples. Bai & Perron (2003) argue that shorter samples exhibiting autocorrelation and heteroskedaticity calls for larger trimmings in percentage of the sample size.

time lag is present between the *de jure* reforms and the detected economic outcome of those. A change in policy does not manifest itself immediately as an economic outcome. That is, there is evidence of 'rigid institutions' (Acemoglu et al. 2006). In most cases a time lag of one year is present (see appendix 1a).

Table 2: Identified privatisations

Country	**Sample period**	**Detected Privatisations**	***De facto* privatisations**
Australia	1969-2009		
Austria	1960-2010	1968; 1988	1968; 1988
Belgium	1995-2010		
Canada	1970-2010	1985; 1993	1985; 1993
Denmark	1971-2010	1983; 1989	
Finland	1960-2010	1992	1992
France	1990-2010	2005	2005
Germany	1970-2010	1982; 1997; 2003	1997; 2003
Greece	1987-2010	1993	1993
Iceland	1960-2010	1983; 1991; 1996	1991
Ireland	1960-2010	1984	
Italy	1988-2010	1993	1993
Japan	1960-2009		
Luxembourg	1975-2009	1981; 1999	1999
Netherlands	1972-2002	1997	1997
New Zealand	1970-2010	1989	1989
Norway	1960-2010	1979; 1988	1988
Portugal	1970-2010	1981	
Spain	1960-2010	1988; 1994	1988; 1994
Sweden	1970-2010	1984; 1990; 1995; 2000	1995; 2000
Switzerland	1985-2010		
United Kingdom	1960-2010	1981; 1986; 1996	1981; 1986
United States	1960-2010		

Data source for detected privatisations: OECD.org, Economic Outlook nr. 90. West German data is used prior 1990 for Germany.
Data source for validated privatisations: HSiT (Healthcare Systems in Transition) country reports (see appendix for details).

The methodology used to detect privatisations generates a binary variable. Years in which a privatisation is detected are coded as 1, the remaining years as 0. This constitutes the dependent variable used in the estimations that follow.

3.2. Explanatory variables

To test hypothesis 1 three commonly applied indicators of economic crisis are used. First, if public debt is at high levels it is reasonable to expect that this positively will impact the pressure to privatise fiscally burdensome sectors of the economy, such as healthcare financing. However, what matters is not the level of public debt as such, but whether financial markets judge the debt to be sustainable. This we can call a sovereign debt crisis effect. The dummy variable *debt crisis* is used to capture this effect. It is defined as an interest rate on long-term government debt in country i at time t, above 11.42 = the sample mean plus one standard deviation. Debt crisis could be captured in other ways, for example using the Laeven & Valencia (2008) sovereign debt crisis indicator. However, in their dataset a debt crisis is defined as actual default or restructuring of public debt. This definition is too strict as it is reasonable to expect that government will react to high interest on government debt with reforms, before actually defaulting on their debt. Furthermore, only very few of these instances are observed in OECD countries in the sample period using Laeven & Valencia (2008).

Second, poor macroeconomic performance is another commonly used measure for economic crisis. Economic growth rates capture this effect. Specifically the dummy variable *annual recession* is used. It is defined as a whole year in which accumulated economic growth is negative. The reason not to use the common definition of a recession (two or more consecutive quarters of negative economic growth) is that we want to capture severe economic growth crises. A country might experience a recession during a period of a year, but still end up with a positive growth rate annually. When the crisis is only mild and/or transitory it might not call for strong policy action.

A third variable commonly related to economic crisis is the unemployment rate. The dummy variable *job crisis* is used to capture this. It is defined as an unemployment rate in country i at time t, above 9.57 = the sample mean plus one standard deviation. Ideally, one would control for differences in structural unemployment. However, structural unemployment varies significantly over time and no annual estimates for the sample period exist (see Turner et al. 2001). In most OECD countries, an unemployment rate above 9.5% would be viewed as an indicator of severe economic crisis.

All three measures for economic crises are expected to generate positive significant coefficients.

<u>To test hypothesis 2</u> the *Government ideology* index proposed by Potrafke (2009) is used. This measures ideology on a left-right scale with five points, the measure goes from 1-5; where 1 is right-wing politicians in 2/3 or more of the parliament or cabinet seats, 2 is right-wing politicians in between 1/3 and 2/3 of the seats, 3 is if the share of centre parties is 50%, or if the left-wing and right-wing parties form a coalition government that is not dominated by one side or the other. 4 and 5 is symmetric to 1 and 2, counted in seats taken by left-wing politicians in the parliament or cabinet. Years in which the government has changed are labelled according to the government that was in office for the longest time that year. The measure is consistent over time, but it does not attempt to capture links between sister parties in different countries (Potrafke 2009). According to hypothesis 2, a positive significant coefficient is expected.

<u>To test hypothesis 3</u> the *Government fractionalization* measure from the World-Bank database on political institutions is used. It measures the probability that two randomly chosen deputies from the government parties will be of different parties (Beck et al. 2001). That is, how many different parties are parts of the coalition and how large are the individual coalition parties relative to all seats taken by the coalition. When the measure increases the degree of government fractionalization increases. According to hypothesis 3, a negative significant coefficient is expected.

<u>To test hypothesis 4</u> the variable *Major Cabinet Changes* from (Banks 2011) is used. It addresses the issue of whether a reform is more likely when a new government, or a government executive is in

place. The variable is defined as the number of times in a year that a new premier is named and/or new ministers assume 50% of the cabinet posts. According to hypothesis 4, a positive significant coefficient is expected.

Table 3: Hypothesis summary table

Hypothesis	**Variables used**	**Expected sign**
1. The likelihood of healthcare financing privatisations increases when the economy is in a state of crisis	Debt crisis, annual recession and job crisis	+
2. The likelihood of healthcare privatisation increases when a left-wing government is in office	Government ideology, The Potrafke index	+
3. The likelihood of healthcare privatisation decreases when governments are more politically fractionalised	Fragmentation of governments, The World-Bank.	**–**
4. The likelihood of healthcare privatisation increases when a newly elected government is in office	Major cabinet changes from Banks (2011)	+

3.3 Control variables

To build a trustworthy empirical model some control variables are crucial. Factors impacting the likelihood of healthcare financing privatisations through costs and funding, while being correlated with the main variables of interest must be included. Shirley and Walsh (2000 p. 44) assert: *"Instead of maximizing its own rents and power, the government places a priority on efficiency. It can be argued that governments that engage in privatisation are not the ones that seek only rents and power."* Specifically, demand- and supply-side factors for healthcare are expected to affect the efficiency of the sector, and hence the likelihood of privatisations. On the demand side an aging population will affect the level of spending of the healthcare sector adversely due to increased demand for healthcare (Oxley & MacFarlan 1995). An aging population may therefore have a positive impact on the likelihood of privatisation. Demographic factors are approximated by the percentage of the population over 65 years of age. On the supply side, technological change, in the form of new drugs, equipment and techniques, medical personnel and facilities, are expected to affect efficiency (Oxley & MacFarlan 1995). However, the effect of technological change is unclear. Supply of new technology will increase costs, but at the same time make medical treatment more effective. Nonetheless, it is expected to affect the likelihood of privatisation. Technological change is approximated by *potential years of life lost,* which is closely but inversely related to life expectancy at birth. Furthermore, public financing can take place through inflation. Governments can use their power to issue new money to finance fiscal expenditures. Because inflation is significantly correlated with the government interest rate, it needs to be controlled for. This concludes the political and economic variables included in the empirical model. Table 4 shows summary statistics and data sources and table 5 presents the correlation matrix of the variables described.

Table 4: Summary statistics and data source

Variable	**obs.**	**mean**	**st.dev.**	**min.**	**max.**	**source**
Government ideology	1101	2.86	0.88	1	4	Potrafke (2009)
Government fractionalisation	789	0.28	0.28	0	0.83	Beck et al. (2001)
Major cabinet changes	715	0.38	0.54	0	3	Banks (2011)
Government interest rate, long-term debt	995	7.66	3.76	1.00	29.74	OECD.org
Growth rate	706	3.11	2.51	-6.37	13.06	OECD.org
Unemployment rate	719	5.94	3.63	0.08	19.11	OECD.org
Inflation rate	718	5.91	7.17	-0.90	83.95	OECD.org
Demographics	1173	12.96	2.87	5.70	23.00	OECD.org
Technological change	1060	75.72	3.41	62.7	83	OECD.org

All available observations for the 23 OECD countries between 1960-2010 have been used.

As shown in table 1a in the appendix, the policy change in the vast majority of cases occurs one year prior to the detected privatisation. Political variables are therefore lagged one year, so that the political conditions at the time of policy change match. Economic variables are not lagged. The reason is that economic conditions matter after the reform has passed the legislature. If, for example, an economic crisis ends shortly after a reform passed, then it is likely that the reform will be reversed or not even implemented.

Table 5: Correlation between explanatory variables

Variable	I	II	III	IV	V	VI	VII	IIX	IX
Ideology$_{t-1}$	1.00								
Fractionalisation$_{t-1}$	-0.144	1.00							
Major Cabinet changes$_{t-1}$	-0.010	-0.070	1.00						
Gov. interest rate	-0.025	0.017	0.088	1.00					
Growth rate	0.001	-0.017	-0.037	-0.055	1.00				
Unemployment rate	0.065	-0.161	0.051	-0.073	-0.037	1.00			
Inflation rate	-0.023	0.063	0.067	0.878	-0.052	-0.206	1.00		
Demographics	0.195	0.063	-0.010	-0.418	-0.165	0.111	-0.387	1.00	
Technological change	0.009	-0.220	0.049	0.499	-0.053	-0.015	0.408	-0.418	1.00

Number of observations: 539

The sample period differs across countries either because of missing data for the dependent or explanatory variables. There are basically two options to proceed in that case, either make the panel balanced by excluding countries or certain periods, or perform the estimations with an unbalanced panel. The main question is whether observations are missing in a random way, i.e. is selection attrition exogenous? If observations are deleted to obtain a balanced panel we lose observations in a way that is equally random/non-random as performing the estimations with an unbalanced panel. Either some countries have to be deleted, or the time dimension will be severely restricted. Therefore, the estimations are done using the unbalanced panel in order to maximise the number of observations under the assumption the attrition is exogenous. The trimming parameter h=5 used when running the B&P-filter has the implication that privatisations cannot be identified at the first and last four observations. Therefore, these are not used in the estimation, as this would bias the results.

3.5 Estimation strategy

The methodology presented in section 3.1 is used to identify healthcare financing privatisations. The identified privatisations are used as dependent variable in limited dependent variable logistic regressions. In the case of a binary dependent variable the observational rule can be explained by an underlying latent variable, namely $y^*_{it} > 0$ if we observe a privatisation, i.e. $y = 1$. In terms of the underlying latent variable this means that the government only decides to privatise when the (expected) benefit from doing so is positive, if $y^*_{it} \leq 0$ we will not observe a privatisation, i.e. $y = 0$. When the random-effects estimator is applied $y^*_{it} = x'_{it}\beta + \varepsilon_{it}$ is interpreted as the government's inclination to privatise. x_{it} is a vector of independent variables, β is a vector of coefficients to be estimated and ε_{it} is a vector of random errors. The probability of privatisation is: $pr(y = 1) \rightarrow pr(y^*_{it} > 0) \rightarrow pr(\varepsilon_{it} > -x'_{it}\beta) \rightarrow F(x'_{it}\beta)$ where F is the logistic cumulative distribution function that ensures that probability of reform is bounded between zero and one.

The main question is whether the included explanatory variables are correlated with time invariant country specific effects. If this is the case the random effects estimator is inconsistent due to omitted variable bias. However, if there is no correlation between the explanatory variables and the country specific effects the random effects estimator is consistent and efficient, and hence the preferred estimator (Baltagi 2008).

The Hausman-test is commonly used to test whether it can be assumed that the explanatory variables and the fixed-effects are uncorrelated. In this specific case the Hausman-test often fails due to a non-positive definite differenced variance matrix. Therefore, an alternative test is applied. Specifically, we create country specific averages over time for each explanatory variable, and then use these as additional controls in a random-effects model. A Wald-test can then be used to for joint significance of the averages. This also implies that we assume that the fixed-effects are a linear function of the explanatory variables (Mundlak 1978).

Furthermore, duration dependence in panel models with a binary dependent variable is a well-known problem. If it is not taken into consideration wrong inference is likely. Maximum likelihood estimators rely on the assumption that the probability of privatisation within countries is independent over time. Beck et al. (1998) show that time-series cross-sectional data (panel data) with a binary dependent variable is identical to grouped duration data, and propose a simple method by which the temporal dependence can be tested for and corrected. The method is based on the construction of a set of dummy variables counting the length of the spell of no privatisation at every observation, counting from the last year of privatisation. The intuition is that the length of the spell has an impact on the probability that a privatisation will occur. Including these spell dummies has a serious drawback; they take up many degrees of freedom. To mitigate this problem, Beck et al. (1998) propose the construction of three cubic splines. These are polynomial functions that mimic the spell dummies by creating a smoothened function of the otherwise non-smooth function for duration dependence. Additionally, they propose to include a variable that counts the number of previous privatisations, and a variable that counts the length of the spell since the previous privatisation. All three suggestions are included in the estimations (see also Mierau et al. 2007).

4. Empirical results

Table 6 shows the results of the baseline random-effects estimates. In the baseline specification the validated detected (de facto) privatisations are used as dependent variable. Model (1) includes the variables testing for the 'economic crises induce reform hypothesis', the economic control variables and the variables modelling duration dependence. The variables testing the political hypotheses are added individually in models (2), (3) and (4). In model (5) all variables are included. The main finding is that the main trigger of healthcare financing privatisations is economic crises. Job crisis, debt crisis and annual recession all have a positive and significant impact on the likelihood of healthcare financing privatisations. These findings are consistent in all model specifications. Furthermore, there is no evidence that political factors impact the likelihood of healthcare financing privatisations. Neither, ideology, government fractionalisation or new governments are significantly different from zero in any of the specifications. The economic controls are jointly significant at around the 5% level in all specifications. The same holds for the duration variables, this provides evidence that duration dependence should be taken into account. The number of observations differs from model (1) to (5). As explained, this is caused by the absence of data on the political variables. The p-value of the Wald-test of the Mundlak (1978) averages provides evidence that the random-effects specification is consistent, and hence the most efficient estimator.

Table 6: Baseline specification, random-effects estimates.

Dependent variable: *De facto* privatisations

VARIABLES	(1)	(2)	(3)	(4)	(5)
Job crisis	1.575***	1.566***	1.511***	1.576***	1.527***
	(0.004)	(0.004)	(0.006)	(0.004)	(0.006)
Debt crisis	1.851**	1.778**	1.790**	1.857**	1.698*
	(0.032)	(0.038)	(0.040)	(0.032)	(0.052)
Annual recession	1.727***	1.796***	1.731***	1.726***	1.790***
	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)
Government ideology$_{t-1}$		0.229			0.222
		(0.432)			(0.473)
Government fractionalisation$_{t-1}$			-0.398		-0.137
			(0.709)		(0.904)
Major cabinet changes$_{t-1}$				-0.050	-0.064
				(0.925)	(0.903)
Inflation	-0.271**	-0.272**	-0.276**	-0.271**	-0.273**

	(0.036)	(0.035)	(0.035)	(0.036)	(0.039)
Demographics	0.101	0.097	0.107	0.101	0.097
	(0.421)	(0.443)	(0.395)	(0.419)	(0.451)
Technological change	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.368)	(0.442)	(0.445)	(0.372)	(0.565)
Spline 1	0.023*	0.021	0.022*	0.022*	0.021
	(0.078)	(0.105)	(0.089)	(0.092)	(0.107)
Spline 2	-0.016*	-0.015*	-0.016*	-0.016*	-0.015*
	(0.061)	(0.081)	(0.068)	(0.070)	(0.083)
Spline 3	0.005*	0.005*	0.005*	0.005*	0.005*
	(0.052)	(0.066)	(0.055)	(0.056)	(0.068)
Number of previous privatisations	-0.659	-0.643	-0.785	-0.667	-0.694
	(0.227)	(0.243)	(0.205)	(0.222)	(0.270)
Time since last privatisation	0.769	0.719	0.734	0.739	0.707
	(0.109)	(0.142)	(0.134)	(0.132)	(0.149)
Constant	-5.335*	-6.088*	-5.137	-5.235	-6.071*
	(0.097)	(0.074)	(0.120)	(0.105)	(0.095)
Observations	634	615	539	619	539
Number of countries	23	23	23	23	23
Log-likelihood	-64.51	-64.16	-64.23	-64.47	-63.96
Wald-test of Mundlak averages	(0.896)	(0.950)	(0.654)	(0.806)	(0.545)

*** p<0.01, ** p<0.05, * p<0.1. P-value in parentheses

5. Sensitivity analysis

In this section it is analysed whether the results are sensitive to relaxing the condition about what constitutes a privatisation. Also, it is analysed whether less severe economic crises impact the evidence in favour of the 'economic crises induce reform hypothesis'. This provides a test of whether the hypothesis is falsifiable. Furthermore, other measures of the political variables are included to analyse whether the results are driven by the used variables.

In the first part of the sensitivity analysis the assumption that privatisations have to be confirmed by evidence of *de jure* reforms is relaxed. That is, we rely solely on the privatisations detected by the statistical filter, see column 3 in table 2 above.[5] This has some noteworthy implications. First, some of the privatisations used in the estimations might not be the result of planned policy. They could be the result of other factors such as exogenous shifts in consumer preferences or relative price movements. If that is the case we expect the precision of the estimates to decline.

Table 7: Privatisations not validated as policy-induced, random-effects estimates

Dependent variable: Detected privatisations

VARIABLES	(1)	(2)	(3)	(4)	(5)
Job crisis	0.907**	0.901*	0.942*	0.904*	0.965**
	(0.049)	(0.050)	(0.050)	(0.050)	(0.046)
Debt crisis	1.165**	1.156**	1.003*	1.165**	1.016*
	(0.037)	(0.038)	(0.084)	(0.037)	(0.080)
Annual recession	1.030**	1.090**	1.035**	1.032**	1.113**
	(0.048)	(0.039)	(0.047)	(0.048)	(0.035)

[5] Another interesting endeavour is to analyse what effect it will have to use *de jure* privatisations as dependent variable. However, this is infeasible because it cannot be determined on the basis of *de jure* evidence alone whether a given reform is supposed to result in a privatisation, a nationalisation, or any.

Government ideology$_{t-1}$		0.154			0.194
		(0.499)			(0.417)
Government fractionalisation$_{t-1}$			0.363		0.517
			(0.660)		(0.542)
Major cabinet changes$_{t-1}$				0.047	0.085
				(0.908)	(0.831)
Inflation	0.015	0.015	0.016	0.015	0.015
	(0.532)	(0.532)	(0.493)	(0.536)	(0.518)
Demographics	0.235**	0.225**	0.239**	0.234**	0.229**
	(0.026)	(0.035)	(0.027)	(0.027)	(0.037)
Technological change	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.337)	(0.322)	(0.781)	(0.334)	(0.786)
Spline 1	0.059*	0.058*	0.054*	0.059*	0.053*
	(0.063)	(0.071)	(0.088)	(0.067)	(0.095)
Spline 2	-0.032*	-0.031*	-0.029	-0.032*	-0.028
	(0.071)	(0.081)	(0.105)	(0.076)	(0.116)
Spline 3	0.004	0.004	0.003	0.004	0.003
	(0.162)	(0.181)	(0.241)	(0.165)	(0.270)
Number of previous privatisations	-0.080	-0.065	-0.061	-0.080	-0.021
	(0.795)	(0.831)	(0.848)	(0.797)	(0.948)
Time since last privatisation	1.587**	1.560*	1.501*	1.576*	1.486*
	(0.047)	(0.053)	(0.059)	(0.052)	(0.060)
Constant	-10.129***	-10.398***	-10.779***	-10.099***	-11.360***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Observations	634	615	539	619	539
Number of countries	23	23	23	23	23
Log-likelihood	-97.27	-97.02	-95.99	-97.26	-95.64
Wald-test of Mundlak averages	(0.197)	(0.286)	(0.370)	(0.247)	(0.477)

*** p<0.01, ** p<0.05, * p<0.1. P-value in parentheses

The results confirm the main findings of the baseline model in section 4. As expected the estimates are less precise compared to the baseline results, both in terms of significance of the crisis indicators and the log-likelihood. Also, there is evidence that adverse demographics trigger privatisations and not, as in the baseline case, that high inflation decreases the likelihood of privatisations. The economic controls are jointly significant at around the 5% level in all specifications, the duration variable at the 10% level in all specifications. The p-value of the Wald-test of the Mundlak (1978) averages provides evidence that the random-effects specification is consistent, and hence the most efficient estimator.

It is interesting to investigate whether other measure of economic crisis impact the results. That is, whether measures for less severe economic crises will change the findings. To that end, the unemployment rate, government interest rate and growth rate are included in raw form, i.e. not dummies that are believed to capture severe crisis. Rodrik (1996) argues that reform following crisis is a tautology. The baseline random-effects estimates (table 6) provide support for such a claim since all proxies for economic crises significantly increase the likelihood of healthcare financing privatisation. Including measures that also capture lees severe economic crises shows that below average economic growth does not trigger privatisations (table 8). The remaining results confirm the baseline results. The p-value of the Wald-test of the Mundlak (1978) averages provides evidence that the random-effects specification is consistent, and hence the most efficient estimator. In sum, this provides evidence that the economic crises trigger reform hypothesis is not a tautology. Differences in severity of the crises have different effects. Therefore, the hypothesis can indeed be falsified since the most common indicator of macroeconomic performance only is a significant predictor when designed to capture severe economic crises.

Table 8: Less severe economic crisis, random-effects estimates

Dependent variable: *De facto* privatisations

VARIABLES	(1)	(2)	(3)	(4)	(5)
Unemployment rate	0.165**	0.159**	0.159**	0.165**	0.156**
	(0.012)	(0.015)	(0.017)	(0.011)	(0.019)
Government interest rate	0.300**	0.307**	0.298**	0.305**	0.309**
	(0.012)	(0.011)	(0.015)	(0.012)	(0.014)
Growth rate	-0.125	-0.133	-0.129	-0.129	-0.139
	(0.255)	(0.229)	(0.247)	(0.244)	(0.218)
Government ideology$_{t-1}$		0.215			0.208
		(0.459)			(0.501)
Government fractionalisation$_{t-1}$			-0.365		-0.148
			(0.732)		(0.897)
Major cabinet changes$_{t-1}$				-0.149	-0.177
				(0.775)	(0.737)
Inflation	-0.329**	-0.347**	-0.334**	-0.335**	-0.355**
	(0.021)	(0.019)	(0.022)	(0.021)	(0.021)
Demographics	0.161	0.161	0.167	0.162	0.161
	(0.214)	(0.219)	(0.200)	(0.212)	(0.224)
Technological change	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.210)	(0.271)	(0.233)	(0.203)	(0.312)
Spline 1	0.022*	0.020	0.022*	0.022*	0.021
	(0.083)	(0.114)	(0.097)	(0.096)	(0.113)
Spline 2	-0.015*	-0.014	-0.015*	-0.015*	-0.014
	(0.079)	(0.106)	(0.089)	(0.089)	(0.105)
Spline 3	0.004*	0.004	0.004*	0.004	0.004
	(0.093)	(0.117)	(0.098)	(0.102)	(0.119)
Number of previous privatisations	-0.821	-0.797	-0.931	-0.842	-0.849
	(0.145)	(0.160)	(0.140)	(0.138)	(0.186)
Time since last privatisation	0.807*	0.744	0.767	0.777	0.742
	(0.094)	(0.129)	(0.120)	(0.114)	(0.129)
Constant	-7.761**	-8.420**	-7.433**	-7.585**	-8.223**
	(0.022)	(0.020)	(0.031)	(0.026)	(0.030)
Observations	634	615	539	619	539
Number of countries	23	23	23	23	23
Log-likelihood	-66.93	-66.60	-66.75	-66.84	-66.47
Wald-test of Mundlak averages	(0.710)	(0.715)	(0.619)	(0.757)	(0.663)

*** p<0.01, ** p<0.05, * p<0.1. P-value in parentheses

As additional robustness checks several other commonly used measures for political factors was used. Concerning ideology, the index on *ideological complexion of parliament and government* by Woldendorp et al. (2000), used in many previous studies, was applied. Also, the index of *average complexion of parliament* constructed by Volkerink & de Haan (2001) was applied. They both turned out to be insignificant. Concerning government fractionalisation *the number of spending ministers in* *government* was applied and turned out to be insignificant. Concerning political fractionalisation *maximum ideological distance in the government* was applied (see Mierau et al. 2007) and turned out to be insignificant. Lastly, concerning a new government or government executive, the occurrence of a legislative and executive election from Beck et al. (2001) was applied. These measures also turned out to be insignificant. The significance of the crisis indicators was unaffected by the inclusion of these alternative political variables.

6. Conclusion

This paper examines whether economic and/or political factors impact the likelihood of healthcare financing privatisations in OECD countries, a topic not investigated quantitatively thus far. A possible reason for this lack of research was the absence of a methodology with which gradual reforms in the healthcare financing sector can be identified. A main contribution of this paper is the development of such a methodology.

The proposed methodology, a combination of structural break testing and validation of these breaks using *de jure* reforms, diminishes the probability of committing type I errors when identifying *de facto* reforms by assuring that they are the result of planned policy and are statistically significant at the same time. The sensitivity analysis shows that purely statistical identification of privatisations leads to imprecise estimates possibly caused by type I errors. These errors could be caused by exogenous shifts in consumer preference or relative price movements. Studies in other areas of the reform literature that rely solely on one of the two kinds of data to identify reforms are therefore likely to suffer from measurement error. Hence, the developed methodology should be applied in other areas of the empirical reform literature.

There are two main empirical results. First, economic crises trigger healthcare financing privatisations, especially severe economic crisis. This result is in line with the findings of Drazen & Easterly (2001). It might seem that pure economic factors thrusts reform. However, the underlying explanation for this finding is mainly political.

Second, political variables do not impact the likelihood of privatisations. Political ideology is not found to be a significant predictor of privatisations. Furthermore, the war of attrition hypothesis is often invoked to explain the delay of reform. No evidence is found in favour of this hypothesis either. Lastly, no support is found in favour of the hypothesis that newly elected governments enjoy a period where reform can be launched with lower political costs.

If healthcare financing privatisations are seen as a means to improve fiscal balances the likelihood of achieving this is driven by economic crisis, and not pure political factors. Thus, the current crisis in many OECD countries might provide a window of opportunity to shift the costs of healthcare to the private sector.

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Appendix 1a: Description of data used to identify privatisations

Column 1 and 2 gives the country and sample length. Column 3 shows some basic time series properties. Underlined years in column 4 and 6 are the detected privatisations that can be validated by the qualitative evidence, the remaining cannot. These are also summarised in table 2 above when h=5. h=5 and h=6 indicate the trimming used when running the B&P-filter. Using a trimming equal to five generates the best congruence compared to the qualitative evidence. In most cases the policy change occurred one year prior to the detected privatisation, see the last three columns. Column 5 and 7 shows detected nationalisations.

Country	**Time period**	**Time series properties**	**Detected reforms**				**Policy change: Privatisations**		
			h=5		**h=6**			**h=5**	**h=6**
			priv.	**nat.**	**priv.**	**nat.**	**Qualitative evidence of reforms that affected healthcare financing**	**priv.**	**priv.**
Australia	1969-2009	AR(1) No time trend		1983		1983			
Austria	1960-2010	AR(1) No time trend	<u>1968</u> <u>1988</u>	1974 1980 1996	<u>1967</u> <u>1988</u>	1974 1980 1996	• Civil Servants' Health and Work Accident Insurance Act of 1967 • Act on Social Insurance for the Self-employed in Commerce, Trade and Industry Act on Social Insurance for Farmers Act on Social Insurance for Self-employed Freelancers of 1979 • Employment and Social Security Tribunal Act of 1987 (HSiT 2006)	1967 1987	1967 1987
Belgium	1995-2010	AR(0) No time trend							
Canada	1970-2010	AR(2) No time trend	<u>1985</u> <u>1993</u>	1974	<u>1985</u> <u>1993</u>	1975	• The Canadian Health Act of 1984 denies federal support to provinces that allow extra-billing within their insurance schemes and effectively forbids private or opted-out practitioners from billing beyond provincially mandated fee schedules. • Federal transfers frozen in 1990/1991. (HSiT 2005)	1984 1991	1984 1991
Denmark	1971-2010	AR(1) No time trend	1983 1989	1975 1998	1983 1989	1976 1998	None of the reforms described in HSiT (2007) are near the dates of the identified privatisations.		
Finland	1960-2010	AR(1) No time trend	<u>1992</u>	1964 1969 1974 2002	<u>1991</u>	1965 1974	<u>Milestones in the history of the Finish health care system:</u> • The 90's: Increasing deregulation and emphasis on municipal autonomy. Reforms in the state administration of health care, subsidy reform. Maintaining health care services during and after economic recession (HSiT 2008)	1990's	1990's
France	1990-2010	AR(1) No time trend	<u>2005</u>	1994	<u>2004</u>	1995	• The Health Insurance Act (no. 2004-810, 13 August 2004) • The health insurance vouchers plan of 2004. Aims: (1) extend the population that might benefit from voluntary health insurance, (2) offset the negative impacts of the threshold effect that occurred following the implementation of the universal Health Coverage Act dedicated to the poorest, and (3) start regulating the voluntary health insurance market. The voucher is a grant aiming at lowering the supplementary insurance contract cost. (HSiT 2010)	2004 2005	2004 2005
Germany	1970-2010	AR(2) No time trend	1982 <u>1997</u> <u>2003</u>	1974 1991	<u>2002</u>	1975 1991	• Health Insurance Contribution Rate Exoneration Act of 1996. Represented a shift from cost-containment to an expansion of private payments. Co-payments were viewed as way to put new money into the system. Further strengthened with First and Second Statutory Health Insurance Restructuring Acts of 1997 • Three months after the government was re-elected in September 2002, it introduced two reform bills with ad hoc austerity measures to reduce expenditure. The 12th SGB V Amendment Act froze ambulatory and hospital care budgets for 2003. (HSiT 2004)	1996 1997 2002 2003	2002

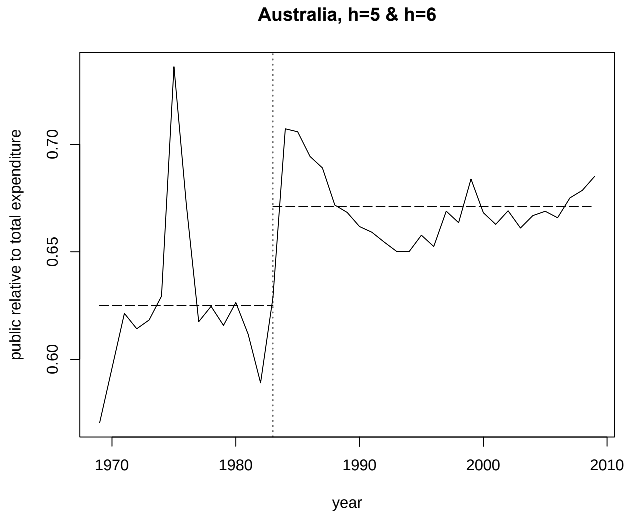
Country	Time period	Time series properties	Detected reforms				Policy change: Privatisations		
			h=5		h=6			h=5	h=6
			priv.	nat.	priv.	nat.	Qualitative evidence of reforms that affected healthcare financing	priv.	priv.
Greece	1987- 2010	AR(1) No time trend	1993	1999	1993	1999	• Law 2071 of 1992: modernization and organization of the health system. The aim was to replace state responsibility with social security and the private sector in the delivery and financing of health services. Incentives to contract with private insurance were given. Co-payment rates for drugs, per diem hospital reimbursement and insurance contributions were increased. Furthermore, fees were introduced for visits to outpatient hospital departments as well as for inpatient admissions. Tax deductions for private insurance premiums were also adopted. A change in government impeded and revoked many of the above measures, Law 2194 of 1994. (HSiT, 2010)	1992	1992
Iceland	1960- 2010	AR(1) No time trend	1983 1991 1996	1965 1971 1976	1983 1992	1965 1971 1977	• The 1990 Health Care Act. Introduction of out-of-pocket user fees. (HSiT 2004)	1990	1990
Ireland	1960- 2010	AR(1) No time trend	1984	1972 1996	1984	1972 1996	None of the major reforms described in HSiT (2009) are near the dates of the identified privatisations.		
Italy	1988- 2010	AR(2) No time trend	1993	2000 2005	1993	2000	• 1992–1993 The government approved the first reform of the national health system (Legislative Decrees 502/1992 and 517/1993). This involved the start of a process of decentralizing health care powers to the regions and a parallel delegation of managerial autonomy to hospitals and local health units. The latter was envisaged within a broader model of internal market reform. During 1992–1993, co-payments were raised. (HSiT 2009)	1992 1993	1992 1993
Japan	1960- 2009	AR(2) No time trend		1969 1974 1988 1994		1969 1975 1988 1994			
Luxembourg	1975- 2009	AR(1) No time trend	1981 1999	1986	1980 1999	1986	• *1998*: *Legislation introducing insurance to cover the cost of long-term care.* The legislation introduced insurance covering home and institutional nursing care, rehabilitation, home aid, nursing appliances, counseling and other support for the elderly and the mentally and physically handicapped. The state pays 45% of the cost of such care the remainder is met by the insured person. (HSiT 1999)	1998	1998
Netherlands	1972- 2002	AR(2) No time trend	1997	1976	1995	1977	• 1992 free choice of fund, end of uniform charges and mandatory contracting. • 1993 prospective financing to funds. • 1994 Van Otterloo Act: low-income pensioners became eligible for sickness funds, however other medium income pensioners lost this right. They now had to rely on private insurance. • 1997. The threshold limit for access to sickness funds for pensioners was significantly raised. At the same time students could no longer be insured jointly under parent insurance. A system of limited user charges for sickness fund enrolees was introduced to give them an incentive to use health services more prudently. (HSiT 2004)	1997	1994
New Zealand	1970- 2010	AR(1) No time trend	1989	1978 2005	1989	1978 2004	• A Public Finance Act 1989 that made sweeping changes to financial management in the public sector. Chief executives were made responsible for financial management; comprehensive new reporting requirements including statements of service performance; and more emphasis on performance indicators were introduced (HSiT 2001).	1989	1989

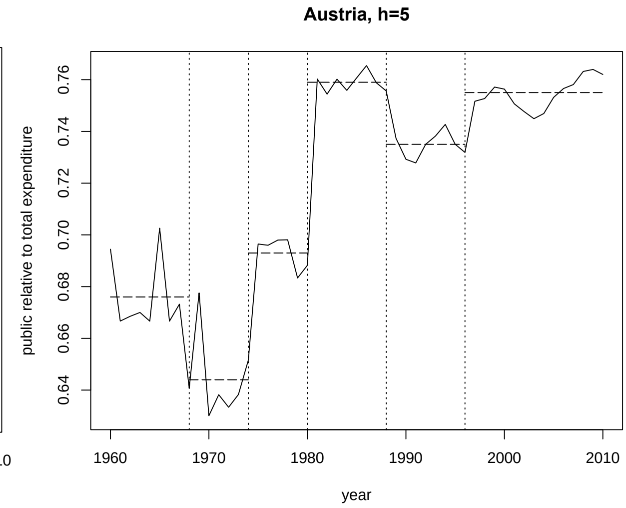
Country	Time period	Time series properties	Detected reforms				Policy change: Privatisations		
			h=5		h=6			h=5	h=6
			priv.	nat.	priv.	nat.	Qualitative evidence of reforms that affected healthcare financing	priv.	priv.
Norway	1960-2010	AR(3) No time trend	1979 1988	1965 1971	1979 1988	1965 1971	• As a result of the Municipalities' Health Care Act (1982), responsibility for the primary health care in Norway was transferred to the municipalities in 1984. The government wanted with this act to coordinate the health and social services at the local level, strengthen these services in relation to institutional care, improve resource utilization, strengthen preventive care, and lay the foundation for better allocation of health care personnel. In 1987, the act was extended to include environmentally oriented health activities. In 1988 the Municipalities Health Care Act was further expanded when the responsibility of the counties' nursing homes was transferred to the municipalities (HSiT, 2000, 2006).	1987 1988	1987 1988
Portugal	1970-2010	AR(1) No time trend	1981	1975 1989 1995	1981	1975 1989 1995	None of the privatisations described in (HSiT 2006) are near the identified date.		
Spain	1960-2010	AR(1) No time trend	1988 1994	1966 1972 1977	1988 1994	1966 1972 1978	• In 1986 the General Health Care Act was approved. The process of devolving central public health powers to the regions was completed. • In 1987 health care powers were devolved to the Autonomous Communities of the Basque Country and Valencia. • In 1993 a selective list of pharmaceuticals was excluded from public funding for the first time. Free choice of GPs and paediatricians was generally introduced (piloted since 1984). • In 1994 an agreement was reached amongst the central government and the special Autonomous Communities on the regional resource allocation system, which involved the rationalisation of a set of previous piecemeal, bilateral agreements, and the commitment to renegotiate the terms of the agreement once every four years. (HSiT 2000)	1986 1987 1993 1994	1986 1987 1993 1994
Sweden	1970-2010	AR(1) No time trend	1984 1990 1995 2000	1974	1984 1991 2000	1975	• In 1994, the Family Doctor Act and the Act on Freedom to Establish Private Practice (1994) was introduced. However, in 1994, the Social Democrats returned to power, and, in June 1995, these two laws were withdrawn before they were fully implemented. Regardless of withdrawal, the Family Doctor Act, together with the Act on Freedom to Establish Private Practice, resulted in increased privatization of primary health care in some counties. • 1998 Patients' share of the drug costs was increased, as a result of a reformed National Drug Benefit Scheme. • 1999 dental reform that meant an increase in patients' co-payments. (HSiT 2005)	1994 1998 1999	1998 1999
Switzerland	1985-2010	AR(1) No time trend included		1990 2000 2005		1990 1998 2004	• The health system has only been reformed in 1994 in the data period. The health insurance law made the purchasing of health insurance compulsory and made significant changes to the systems of subsidies within the system. (HSiT 2000)		
United Kingdom	1960-2010	AR(2) No time trend	1981 1986 1996	1964 1973 2003	1980 1986 1996	1965 1973 2003	• In1979 the government of Margaret Thatcher was elected. It was committed to a program of radical economic and social reform. This government saw public expenditure and state involvement as the source of Britain's economic difficulties and embarked upon a major program of privatization. (there is no specific reference to healthcare financing reform) • In 1985, a Selected List Scheme was introduced restricting the range of medicines that are available through NHS prescriptions. • Between 1995-1999 problems with NHS covering dental services led many to take out private dental care insurance. (This was not the result of a reform as such.) (HSiT, 1999)	1979 1985	1979 1985

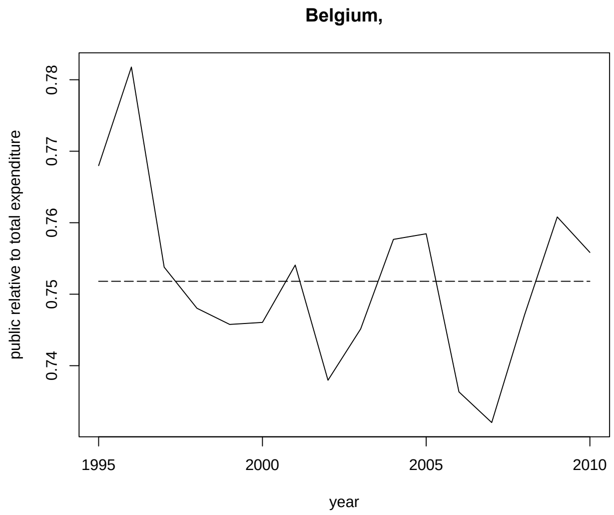
Country	Time period	Time series properties	Detected reforms				Policy change: Privatisations		
			h=5		h=6			h=5	h=6
			priv.	nat.	priv.	nat.	Qualitative evidence of reforms that affected healthcare financing	priv.	priv.
United States	1960-2010	AR(3) No time trend included		1966 1973 1991 2005		1966 1973 1991 2004			

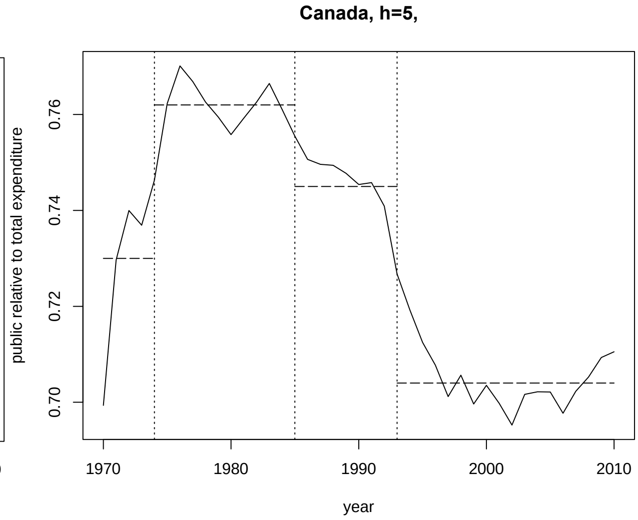
Data source for detected privatisations: OECD.org, Economic Outlook nr. 90. West German data is used prior 1990 for Germany.
Data source for validated privatisations: WHO/ European Observatory on Health Systems and Policies country HSiT (Healthcare Systems in Transition) reports.

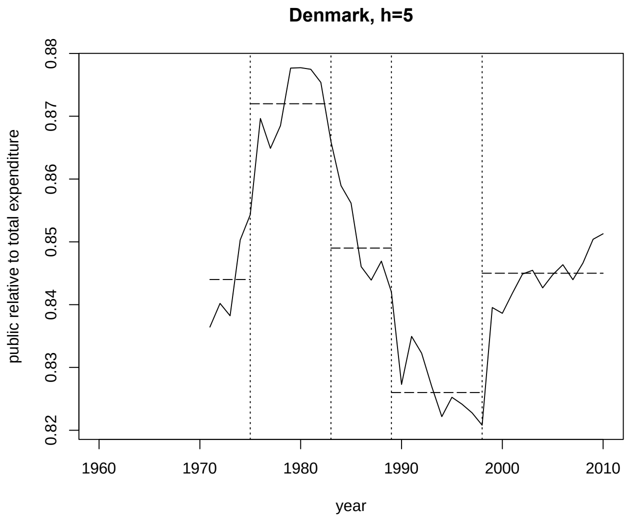
Appendix 2a: Graphs of the individual time series with segments and breaks

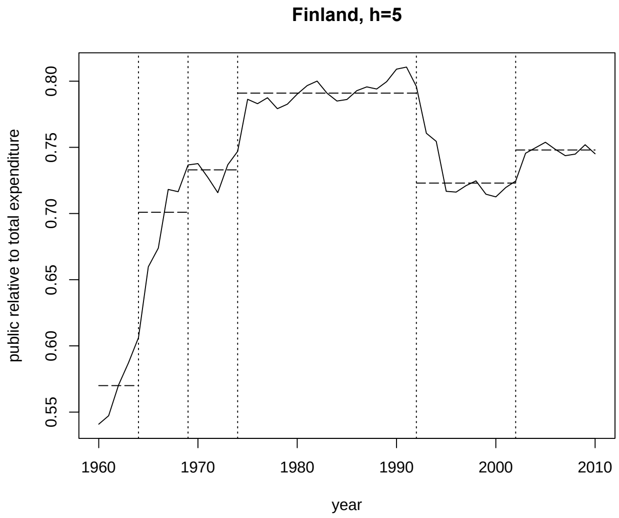




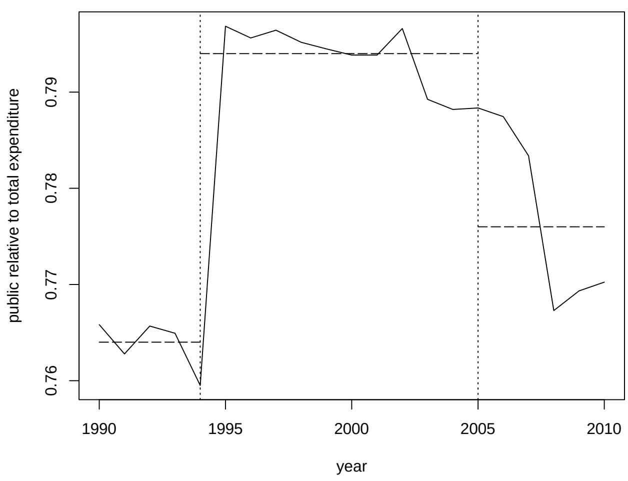






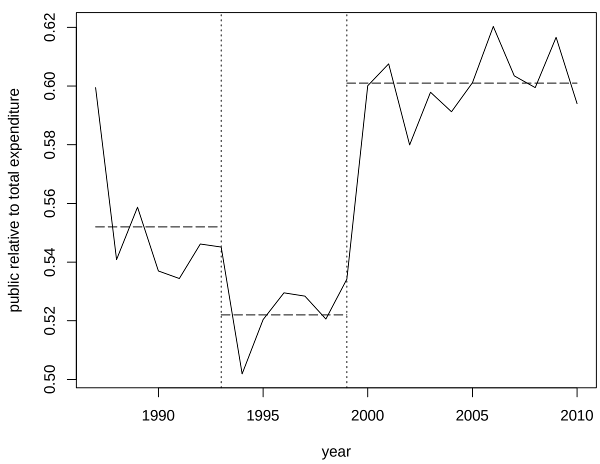


France, h=5



Germany, h=5

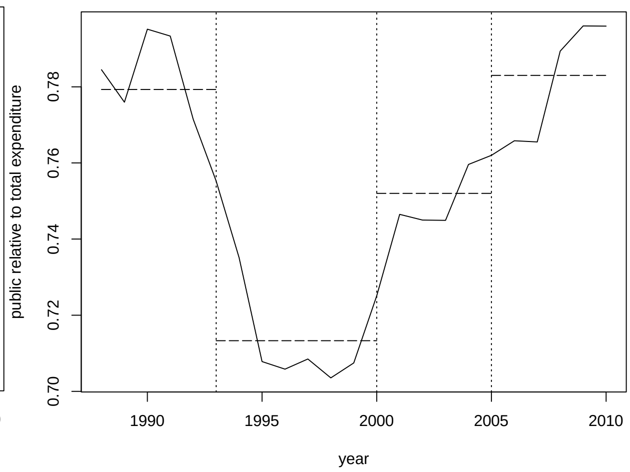
Greece, h=5&6

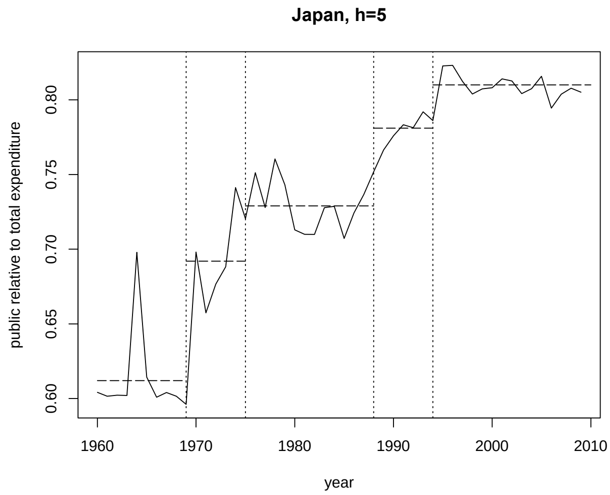


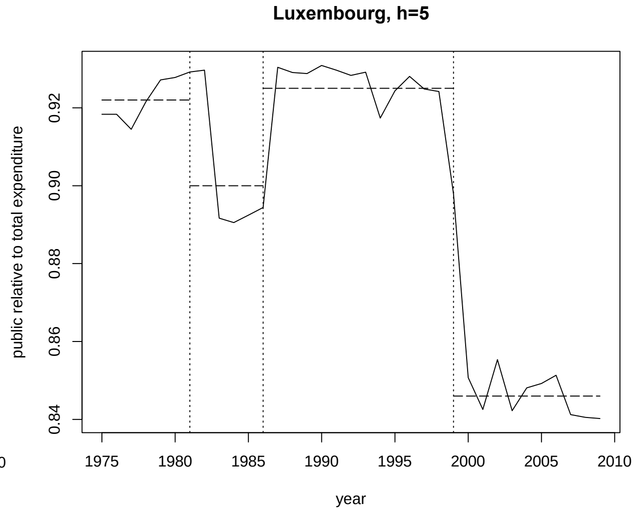
Iceland, h=5

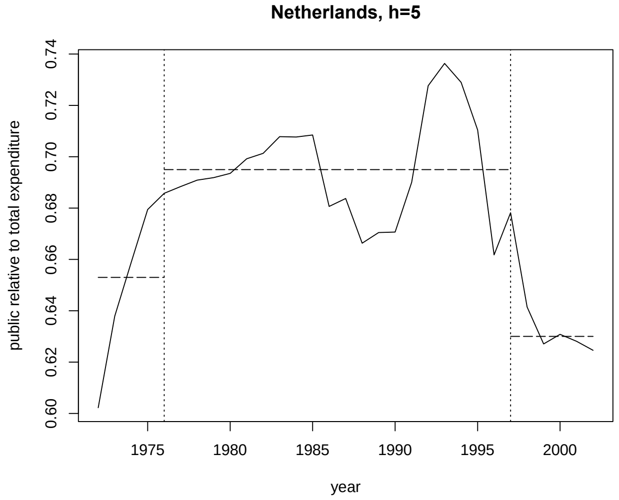
Ireland, h=5&6

Italy, h=5

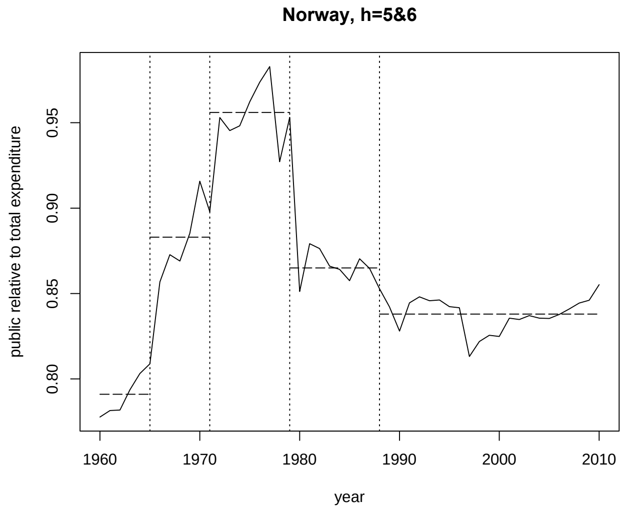


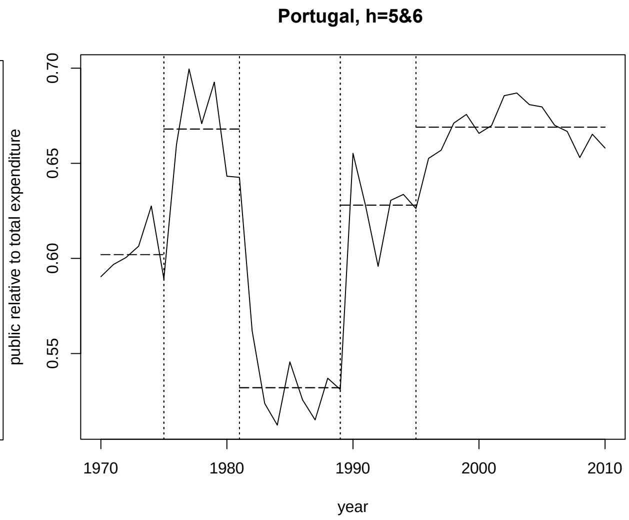

Japan, h=5
public relative to total expenditure
year

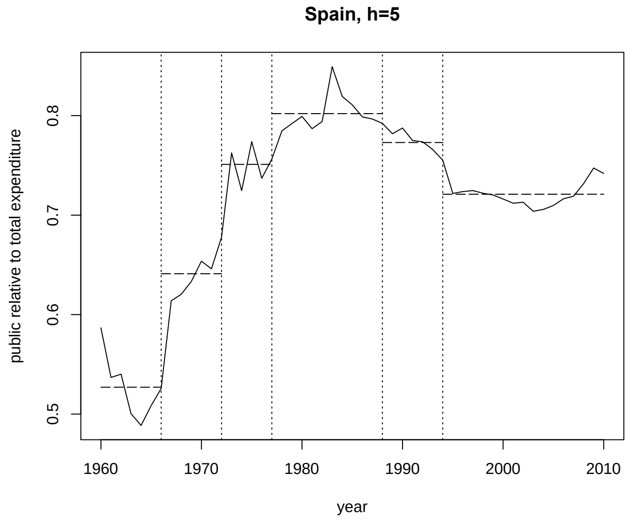

Luxembourg, h=5
public relative to total expenditure
year

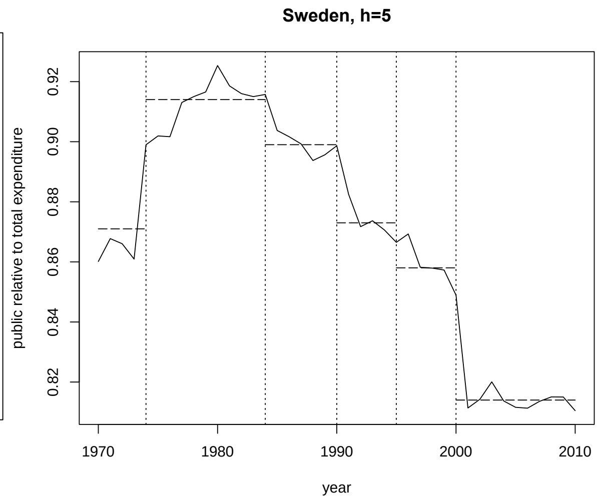

Netherlands, h=5
public relative to total expenditure
year

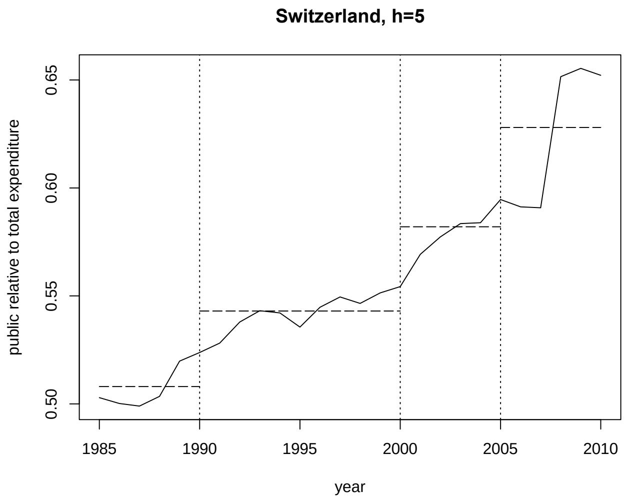

New Zealand, h=5
public relative to total expenditure
year

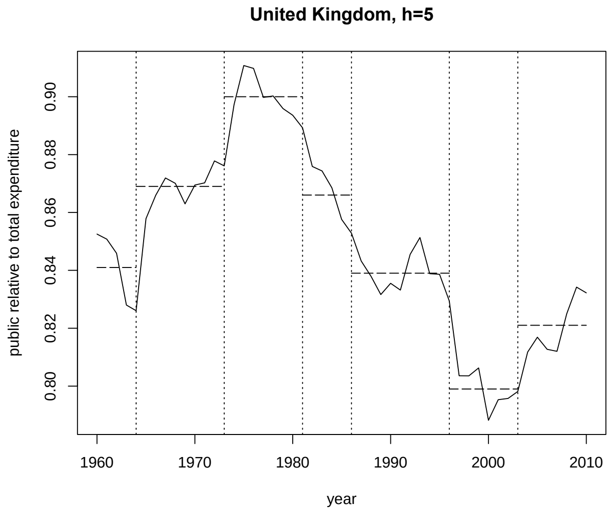

Norway, h=5&6
public relative to total expenditure
year

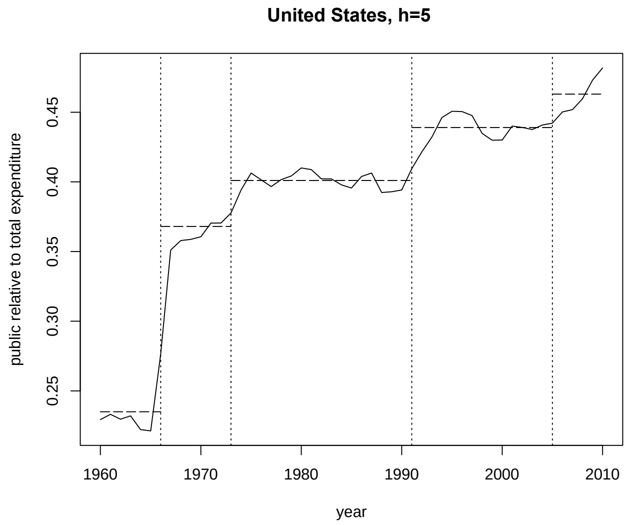

Portugal, h=5&6
public relative to total expenditure
year


Spain, h=5
public relative to total expenditure
0.5
0.6
0.7
0.8
1960
1970
1980
1990
2000
2010
year


Sweden, h=5
public relative to total expenditure
0.82
0.84
0.86
0.88
0.90
0.92
1970
1980
1990
2000
2010
year


Switzerland, h=5
public relative to total expenditure
0.50
0.55
0.60
0.65
1985
1990
1995
2000
2005
2010
year


United Kingdom, h=5
public relative to total expenditure
0.80
0.82
0.84
0.86
0.88
0.90
1960
1970
1980
1990
2000
2010
year


United States, h=5
public relative to total expenditure
0.25
0.30
0.35
0.40
0.45
1960
1970
1980
1990
2000
2010
year

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